be inferred, an indication of the presence of some physical cause tending to increase the rainfall in years of minimum and diminish it in years of maximum solar maculation.

Bankipore, Patna E. D. ARCHIBALD

The Australian Monotremes

IT is certainly news to me, and I believe to most other European naturalists, that *Tachyglossus* and *Ornithorhynchus* occur in Northern Queensland. Perhaps W. E. A. will kindly state, for our information, the exact spots where they have been discovered and their extreme *northern* limit, so far as this has been ascertained.

W. E. A. speaks of an adult female *Echidna* (*sive Tachyglossus*) having "a fine healthy young one in the pouch." Is there not some error here, as the monotremes have, strictly speaking, no marsupial pouch? P. L. S.

WITH reference to the existence of *Tachyglossus* (*olim Echidna*) in North Australia, and the recent discovery of one (or possibly two) species in New Guinea, the following account, which I lighted on a few evenings ago, when looking over an old volume of the *Field*, seems to be of sufficient interest to warrant its transfer to the pages of NATURE. The account in question occurs in an article "A Week at Plain Creek, Queensland," by Mr. E. B. Kennedy, which appeared in the issue of that journal for September 20, 1873. It runs as follows:—" . . . Whilst so engaged we heard our dogs making a tremendous noise, high up the bank in the scrub, and upon going to ascertain the cause found them scratching, yelling, and pulling at a porcupine which was half imbedded in a hole; we were at least ten minutes digging him out with sharp-pointed sticks, such was his tenacity in holding on and burrowing. The quills were not nearly so long as the Cape of Good Hope species (of course a true *Hystrix*), and he differed from that quadruped in having a sort of beak instead of a regular jaw." It is to be regretted that Mr. Kennedy did not preserve his specimen, which was ultimately cooked and eaten! I should have mentioned that Plain Creek lies in 21 lat. S., so that this is certainly the northernmost locality on the Australian continent, where we have certain knowledge that the *Echidna* occurs. As we now know that many North Australian species of birds range also into southern New Guinea, it would hardly be surprising if the *Tachyglossus* of the Fly River and south New Guinea were nothing more than the well-known *Tachyglossus hystrix*. It is to be hoped that this point may soon be solved by the arrival of specimens from both localities. W. A. FORBES

English Names of Wild Flowers and Plants

TO all who are interested in the history of the English language the derivations proposed for the vernacular names of many plants in the Rev. W. Tuckwell's lecture (see NATURE, vol. xvi. p. 385) will be highly appreciated. And even in the few cases where the etymologist may feel doubtful as to the verisimilitude of the suggested pedigree it will for the most part be difficult to propose another with any great confidence.

There is, however, one of these doubtful cases, the derivation of woodruff from wood-roue, in lieu of which I have to offer a conjecture which appears to need no lengthy argument to insure its acceptance.

Is not the ruff of woodruff identical with the riff of sherriff? Is not, in short, the woodruff the wood-reeve, just as the sherriff is the shire-reeve? That the German wald-meister has the same connotation and is applied to the same plant is evidently a striking confirmation of this view, and it would be interesting to know whether the word wald-graf (*i.e.*, wald-ge-raf = wood-y-reeve), or any equivalent form, is to be met with in high or low German literature.

I used to be told by a school-fellow that the way to spell woodruff was—

Double U, double O, double D, E,
Double R, double O, double F, E.

Even under the disguise of woodderrooffe, however, the origin of the word is perceptible.

As regards the main purpose of the Rev. W. Tuckwell's paper, I feel strongly that scientific accuracy is compatible with a much freer use of vernacular words than is customary amongst us, and that their adoption by science teachers would remove a great stumbling-block from the path of learners.

Manningham, September 10 J. WILLIS

Some of the Troubles of John O'Toole respecting Potential Energy

"IT is the people's *right* to demand of their teachers that the information given them shall be, at least, definite and accurate as far as it goes," and "whenever there appears to be a confusion about fundamental principles it is the *duty* of a scientific man to endeavour by all means in his power to remove it." These are the words of one of the teachers.[1] I am one of the people—as indeed, my name testifies, Toole (*Tuathal*) being the Irish equivalent of the Latin *Publius*—and I would now, on behalf of myself and every brother Publius, assert our above "right" in respect of the matter now in hand, and demand the performance by the doctors of their corresponding "duty." Now there is much "confusion about the fundamental principles" of physical Energy in the minds of the public who care about such things; and it is principally, though I admit not entirely,[2] the doctors who are to blame for this. Their own ideas on the subject being so clear and correct they are superior to the phraseology they use respecting it, and they are not injuriously affected thereby; but those who are dependent on that phraseology for their knowledge are in very different case. Let me, as one of the latter, point out some of the perplexities under which we labour from no fault of our own, and which we should be spared if our teachers would only condescend to use their words discreetly and consistently. It may be well to premise that we know the definition of physical Energy, which is—"the power or capacity of performing work;" and that we are not now making any confusion between Energy and force.

The word "potential" has two very different meanings—(1) Of, or belonging to, potency or power; (2) Existing *in posse*, or in possibility, as opposed to existing *in esse*, or in actuality; and the expression, "potential Energy," can have no less than three references or meanings, which we shall mark with A, B, and C; and each meaning has its own proper inconveniences independent of the perplexities arising from their mutual relations.

A.—*Potential E., as meaning "Energy existing* in posse."

The phrase "potential E." is in the first place very generally intended to mean E. existing *in posse*, according to one proper signification of the word "potential." The phrase was first used by Rankine,[3] and apparently in this sense; he contrasted "potential" and "actual" E. This antithesis is still very generally implied and sometimes expressed. Clerk Maxwell tells us[4] (the statement being *repeated* only last year[5]) that "potential E." "signifies the E. which a system has not in actual possession, but only has the power to acquire." Wormell says[6]—"It has been aptly called possible or *potential* E., because it represents the power the body has of acquiring actual or kinetic E." Many of our doctors use the phrase "potential E." without explaining it, and of course, unless there be some particular reason to the contrary, such must be understood to give it, as one of its significations, at least, the original meaning intended by its proposer (or if not they are guilty of a very misleading omission, *utrum horum mavis accipe*); and this is especially undeniable in the case of those who apply the title "actual" to the other type of E. Balfour Stewart, though he seems to have quietly dropped the name "potential,"[7] has really retained the idea implied thereby, for he still habitually calls the other type of E., that of motion, "actual E.," as its *distinguishing* title. Moreover, this idea is involved in other statements, &c., of our teachers. For instance, we occasionally find language used which seems to imply that potential E. must first emerge as actual E. before it can produce work, as by Deschanel,[8] by Dunbar Heath,[9] and by Balfour Stewart.[10] Observe, also, the expression "E. of actual motion," which is frequently used by the last-mentioned doctor,[11] and accepted at least by Tait.[12] "E.

1 Tait Evening lecture during meeting of Brit. Assoc. at Glasgow in 1876. NATURE, Sept. 21, 1876.

2 That brother Publius who wrote the article on Tyndall's "Heat," in *Blackwood's Mag.*, December, 1863, was partly responsible for his own confusion about Energy.

3 *Phil. Mag.*, February, 1853. He says: "All conceivable forms of E. may be distinguished into two kinds, actual or sensible, and potential or latent."

4 "Theory of Heat," p. 91, 1871.

5 "Matter and Motion," p. 81, 1876.

6 "Dynamics," p. 185.

7 At least it never occurs in his "Conserv. of Energy," 1874, though frequently in "Elem. Physics," 1870.

8 "Nat. Phil.," p 78, edition of 1870.

9 "Energy," p. 64. 10 "Elem. Physics," pp. 104–106. But see p. 360.

11 "Conserv. of Energy," p. 25, and elesewhere.

12 "Unseen Univ." (last edition), p. 109, twice.

of actual motion," as a *distinguishing* title, cannot mean anything else than this—that the other E., potential E., is E. of about-to-supervene motion, or that it does not perform work except through the resulting E. of motion. We do not say that these doctors intended what we have mentioned, but their words unquestionably go to convey that impression; and what makes this so particularly mischievous is that poor Publius is already so susceptible to that impression, being prepared for it by the titles "potential" and "actual" E.

I should not be at all surprised if some would try to argue that the phrase "potential E." need not be taken to mean more than simply this, viz., that the E. so called exists in possibility *relatively to the body or system* that may be in question, that its potentiality merely implies that it is absent from and acquirable by that body or system, and not that it is altogether out of actual existence.

1. Now even supposing this to be true, though I have never seen any evidence of it, and even if we should grant this to be a right usage when the *body* or *system* is followed through the history of its changes, it is a wrong usage when, as in a book or chapter on E., Energy is the subject, when it is the conserved E. *itself* which is to be followed through its migrations. Why should this grand conserved E. be stigmatised as merely potential when it does not happen to be in a certain mass? Relatively to that mass it may be some times potential, but relatively to itself it is, as we shall see, always actual.

But we cannot concede that the potentiality of this mode of E. implies merely the above. I believe it is usually intended to mean much more; and, at any rate—whatever those who use the word may intend—it logically involves much more; and this is what poor Publius is chiefly concerned with. Now if we consider the words of Clerk Maxwell quoted above, we shall see that in the case of a separate unconnected system, such a statement coming from him cannot possibly mean that the said E. is in actual existence outside of the system, and is waiting there until the system takes possession of it. If it is not in the system there is nowhere else where it can be; therefore it is not in actual physical existence at all, although connected with existence by some inconceivable paraphysical link. The acquiring of it is a kind of creation of it. Curiously enough Stewart and Tait[1] speak of the "creation" and "annihilation" of both types of E.

2 Here, then, comes our second complaint. E. is "the power of performing work;" therefore potential E., which is intended to be the power of acquiring E., is the power of acquiring the power of doing work. E. is already a potentiality; therefore potential E. is a potentiality which, itself, exists only in potentiality. There is here a double remotion from tangibility, which may be gratifying to the metaphysicians, who rejoice the more the harder the nuts you give them to crack; but poor Publius finds *bonnes bouches* of this sort rather trying to his molars.

3. Potential E., in the present sense, being, as we have seen, undeniably out of actual physical existence, poor P. does not feel that he has gained much when he learns that the sum of the actual and potential E.s of the universe is a constant quantity—for this is the form in which the grand principle of the conservation of E. is usually, or at least frequently, presented to him by the doctors. A rigid physicist, who himself believes in nothing but the physical, teaches poor P. something which compels *him* to stand with one foot on the land of physics and the other in the sea of metaphysics, in order to reach it all. This *teacher* forces poor P. to recognise the metaphysical, while he scorns to do so himself. The combination of the two characters of conductor and of pure finger-post, in the same person puzzles Publius a good deal. Sometimes, when poor Publius thinks that he has grasped the principle in the above form, it seems to him to turn out only a truism, after all; and indeed no less a man than Sir John Herschel sympathised with him in this idea.[2] I am not sure that they are right; they seem to overlook that this potential E., though undeniably out of physical existence, is by some mysterious paraphysical operation, recoverable in its former quantity. However, P. and Sir John are right, so far, that the doctors will sometimes inadvertently allow themselves to present a physical principle of E., which is very far from self-evident, in a form which has all the appearance of a logical truism; *e.g.* when we are told that "the E. exerted is equal to the work performed." P. says I could have told you that from the definition of E., which is "the power of performing work."

4. Potential E. being that which is not had in actual possession by the body (or system) in question, how can that body be "a store of potential E.?"[1] How can the body contain that which is not in it? The doctors should explain this. However, I am glad to find that my cousin Barney was not so wrong, after all, when he complained that Ireland was swarming with absentee landlords.

5. But to pass now from *à posteriori* objections to the phrase "potential E." in the present sense. This potential E. is so called to *distinguish* it from actual E. so called, and yet it is just as immediately and directly efficient in performing its work as actual E. itself, and, therefore, as truly actual as any E. can be. When a certain quantity of potential E. is followed by its equivalent actual E., what is the actual E. of the body but the *direct work* of the potential E. done against the inertia of the body? It is from the doctors themselves, of course, that I learn this. And yet it is very curious to observe how often they shrink from directly stating this, and how ingeniously they will avoid it (one doctor actually denies it). They will sometimes tell you that the potential E. is "*transformed*" into the subsequent actual E. and *v. v.* Sometimes, when they feel that this evasive euphemism is unsuitable to their immediate purpose, they will use what I, with the utmost deference as well as difference, hold to be the proper word, viz., "transfer"; but having made this concession they refuse to proceed further, and shirk telling us from what or to what the transference is made (more of this presently). As we have said the kinetic E. of the moving body is the direct work of the equivalent potential E. that preceded it; and if the work be, as it is, actual, the E. must be so too; as long as we remain in the realm of physics.

But more than this; the potential E. of a mass, as it is expressed, can do other direct work than that of producing actual E. in the mass concerned. Take the case of a clock weight, which is so often adduced, though never, as far as I know, for the purpose of illustrating its own proper lesson. When wound up it has, as we are told, potential E.; but in its descent, while working the clock, it never acquires more than the indefinitely small quantity of actual E. which is due to its excessively slow motion; and this actual E. is doing no work during the descent, since the velocity of descent is uniform. The only work that this actual E. performs is to produce an infinitesimal amount of heat at the instant of the weight's reaching the lowest point of its descent; that is to say, when the clock has stopped. This is only one instance of a whole class of cases in which, as it is expressed, a mass does work by means of its potential E. which exists only in possibility, without ever having any actual E. which it can apply to that work! Moreover there are cases outside of molar physics in which it is not yet known, for certain, whether the E. present is conventionally actual or potential; and yet, in either case, the work is done immediately and directly; and therefore the E. is truly actual whether conventionally so or not. Therefore "potential E.," in the present sense, is a wrong title for this or any mode of E., and this being so, "actual E.," as the *distinguishing title* of the other E., is wrong too; since both are actual.

6. There are a very few of our doctors who use the name "potential E." with another reference solely, and who, as it would appear, designedly abstain from giving it the meaning of "E. existing *in posse*," probably on account of some of the inconveniences we have mentioned; and yet they will use epithets which at least tend somewhat in the same direction. They speak of it as being "E. of repose"[2] (meaning of course reposing E.), as being "of a quiet nature,"[3] "dormant,"[4] "quiescent,"[5] "tranquil,"[6] and "passive"[7] (!), in opposition to the other type of E., which they correspondingly call "active"[8] and "living."[9] Now poor Publius is strongly inclined to think that if *he* had spoken thus they would have said that he had not yet got hold of the precise scientific meaning of E. It seems to him, though he trembles to say it, that although in popular usage the phrases, "quiet," "dormant," &c., and "active energy" may do very well, and convey a correct meaning, viz., that intended by the

1 "Unseen Univ.," p. 114.
2 "Familiar Lectures," p. 469. See Rankine's answer to Herschel, *Phil. Mag.*, February, 1867.

1 Thomson and Tait (*Nat. Phil.* p. 178) virtually say this, but with them pot. E. does not mean E. existing in possibility. So they are all right in doing so.
2 Balfour Stewart, "Cons. of E.," pp. 27, 143. 3 *Op. cit.*, p. 23.
4 Stewart and Tait, "Uns. Univ.," p. 109; also Tait's "Glasgow Lect."
5 *Op. cit.*, p. 111. 6 Do., p. 147. 7 Tait, "Glasgow Lecture.
8 "Uns. Univ.," p. 111; Tait, "Glasg. Lect.," and Tyndall, "Heat," 2nd edition, p. 140. 9 Stewart, "Cons. of E.," p. 27.

speaker, yet that physical E., according to its definition, is not capable of having such epithets applied to it, except in senses which are not intended by those doctors. Active E. would not be E. or "the *power of performing* work," it would be rather the *performing* of that work. "Active E." being thus incorrect, its above antithetics, or approximate antithetics, are incongruous expressions, or else have meanings different from what is intended. If "reposing," "dormant," "quiescent" E. have any meaning, it is that of "*unavailable* E." If "quiet" and "tranquil" E. have any meaning, it is that of E. spending itself slowly and equably. Poor P. thinks that the expression, "passive E.," would sound very like a bull, whether used in a tap-room or in a lecture theatre. He dares not entertain the suspicion that these expressions had their origin in a momentary, latent, unconscious confusion between kinetic E. and action in the minds of the writers; but he knows that they are eminently calculated to cause a chronic intentional muddling of them both together in his own brain-pan.

Dublin (*To be continued.*) X.

On the Supposed Action of Light on Combustion

THE results obtained in the experiments mentioned by M. C. Tomlinson are to be attributed to the elevation of temperature of the candles exposed to solar *light* and *heat*.

The influence of light on combustion has been mistaken for the action of heat, which, in this instance, seems to have accelerated combustion, and in other instances *retards it* by increasing *the heat of the air* and diminishing the *draft*. That is why the sun shining over chimney-pots is said to cause smoke; it diminishes the ascensional speed of the air through the pipe.

Jersey, September 2 G. SAVARY

[On referring to Mr. Tomlinson's paper we find that out of four trials, with a number of candles to each, there was a greater consumption of material in the first and fourth trials in the light than in the dark; and in the second and third trials the consumption was greater in the dark than in the light; but in any case the difference was so small, amounting only to from two to seven grains per hour, that it may fairly be referred to accidental circumstances, such as differences in temperature, in currents of air, and in the composition and matter of the candles. Some of the trials were made in the diffused light of day, and in all the trials the differences in temperature between the dark and the light spaces were but small.—ED.]

OUR ASTRONOMICAL COLUMN

THE SATELLITES OF MARS.—The following ephemeris of the outer satellite is deduced from the elements given in this column last week, except that the daily motion is corrected to 285° 5′·147 by observations to Sept. 16 :—

	At 8h. om.		At 10h. om. G.M.T.	
	Pos.	Dist.	Pos.	Dist.
	°	″	°	″
Sept. 24	247	75	237	61
,, 25	280	49	276	68
,, 26	52	54	26	34
,, 27	83	70	75	77
,, 28	192	29	129	31
,, 29	252	76	243	68
,, 30	297	35	274	55
Oct. 1	60	63	44	44
,, 2	90	59	80	71
,, 3	217	38	167	25
,, 4	257	72	249	70
,, 5	328	26	286	41

For the inner satellite the following elements may be taken as representing closely the Washington measures, August 17–20 :—

Passage of Ascending Node, August 17·89788 G.M.T.

Longitude of node	82° 48′
Inclination of orbit	25 24
Period of revolution	0·31841 days.
Log. radius of orbit at mean distance of Mars from the sun	0·9286

They show the following differences of the calculated angles from those observed :—

Aug. 17	− 0°·1	Aug. 19	+ 1°·9
,, 18	− 0·7	,, 20	+ 0·1

The outer satellite has been observed on several nights by A. A. Common, Esq., of Ealing, with an 18-inch silver-on-glass reflector. On the 15th and 16th inst. excellent measures were made with this instrument, by means of which the period of revolution was corrected before calculating the above ephemeris. Mr. Common has stated to the writer that the satellite is ruddy, even more deeply-coloured than the body of the planet. It has also been observed on several occasions by M. M. Henry at the Observatory of Paris.

Employing Kaiser's value of the mean diameter of Mars at distance unity (9″·472) it results that the inner satellite is distant from the centre of the primary 2·730 and the outer one 6·846 semi-diameters. As seen from the inner satellite the globe of Mars will subtend an angle of 40°, and as seen from the outer satellite, one of about 16°. The orbital motions per minute are respectively seventy-nine miles and fifty miles. Our own moon has a mean orbital velocity of thirty-eight miles per minute.

THE SATELLITE OF NEPTUNE.—The subjoined ephemeris is derived from Prof. Newcomb's tables :—

At 13h. Greenwich M.T.

	Pos.	Dist.		Pos.	Dist.
	°	″		°	″
Sept. 30	30	15·2	Oct. 9	25	13·6
Oct. 1	314	5·7	,, 10	101	6·3
,, 2	231	14·9	,, 11	46	16·2
,, 3	209	14·7	,, 12	23	13·0
,, 4	122	5·7	,, 13	294	7·5
,, 5	49	15·4	,, 14	225	16·5
,, 6	27	14·2	,, 15	201	12·3
,, 7	291	5·9	,, 16	86	7·5
,, 8	228	15·8	,, 17	44	16·7

The motion of this satellite is *retrograde* both with reference to the equator and to the ecliptic, and thus it presents the most decided case of retrograde motion in the planetary system; the motion of the satellites of Uranus, though retrograde upon the ecliptic, is direct upon the equator. For 1877 we have for the satellite of Neptune, from Prof. Newcomb's investigation—

	For Equator.	For Ecliptic.
Node	183° 3′	184° 33′
Inclination	121 42	145 13

Adopting the mean of Mr. Lassell's and Mr. Marth's measures of the diameter of Neptune, taken at Malta in 1864-65, as the most reliable value hitherto published, we find that Prof. Newcomb's mean angular distance of the satellite from Neptune corresponds to a true distance of 14·552 semi-diameters of the primary (or about 219,000 miles), which will therefore present an angular diameter of rather less than 8″ as viewed from the satellite. The period of revolution being 5·8769 days, the mean orbital velocity of the satellite is 162 miles per minute.

THE BINARY α CENTAURI.—Mr. Gill has found time to measure this fine star with Lord Lindsay's heliometer at his present station, Mars Bay, Ascension, on four nights between July 22 and August 5. The distance of the components was then little over two seconds, the bright star preceding. The measures are evidently difficult from the magnitude and closeness of the stars, the separate night's results differing by more than 10°; but Mr. Gill will doubtless establish an important epoch, and we may hope at the end of the year to have something like reliable elements of this the most interesting of all the revolving double stars.

METEORIC ASTRONOMY.—The second part of the publications of the Royal Observatory at Münster has appeared, and is entitled, "Resultate der in den 43 Jahren 1833-1875 angestellten Sternschnuppen-Beobachtungen, von Dr. Eduard Heis." It was close upon completion at the time of Dr. Heis's decease on June 30, the revision of the final sheets having been undertaken by one of his pupils. The work contains the times of occurrence and